

SCIENCE

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Why Have the Old Rods Failed?

When lightning-rods were first proposed, the science of energetics was entirely undeveloped; that is to say, in the middle of the last century scientific men had not come to recognize the fact that the different forms of energy—heat, electricity, mechanical power, etc.—were convertible one into the other, and that each could produce just so much of each of the other forms, and no more. The doctrine of the conservation and correlation of energy was first clearly worked out in the early part of this century. There were, however, some facts known in regard to electricity a hundred and forty years ago; and among these were the attracting power of points for an electric spark, and the conducting power of metals. Lightning-rods were therefore introduced with the idea that the electricity existing in the lightning-discharge could be conveyed around the building which it was proposed to protect, and that the building would thus be saved.

The question as to dissipation of the energy involved was entirely ignored, naturally; and from that time to this, in spite of the best endeavors of those interested, lightning-rods constructed in accordance with Franklin's principle have not furnished satisfactory protection. The reason for this is apparent when it is considered that the electrical energy existing in the atmosphere before the discharge, or, more exactly, in the column of dielectric from the cloud to the earth, above referred to, reaches its maximum value on the surface of the conductors that chance to be within the column of dielectric; so that the greatest display of energy will be on the surface of the very lightning-rods that were meant to protect, and damage results, as so often proves to be the case.

It will be understood, of course, that this display of energy on the surface of the old lightning-rods is aided by their being more or less insulated from the earth, but in any event the very existence of such a mass of metal as an old lightning-rod can only tend to produce a disastrous dissipation of electrical energy upon its surface,—“to draw the lightning,” as it is so commonly put.

Is there a Better Means of Protection?

Having cleared our minds, therefore, of any idea of conducting electricity, and keeping clearly in view the fact that in providing protection against lightning we must furnish some means by which the electrical energy may be harmlessly dissipated, the question arises, “Can an improved form be given to the rod so that it shall aid in this dissipation?”

As the electrical energy involved manifests itself on the surface of conductors, the improved rod should be metallic; but, instead of making a large rod, suppose that we make it comparatively small in size, so that the total amount of metal running from the top of the house to some point a little below the foundations shall not exceed one pound. Suppose, again, that we introduce numerous insulating joints in this rod. We shall then have a rod that experience shows will be readily destroyed,—will be readily dissipated,—when a discharge takes place; and it will be evident, that, so far as the electrical energy is consumed in doing this, there will be the less to do other damage.

The only point that remains to be proved as to the utility of such a rod is to show that the dissipation of such a conductor does not tend to injure other bodies in its immediate vicinity. On this point I can only say that I have found no case where such a conductor (for instance, a bell wire) has been dissipated, even if resting against a plastered wall, where there has been any material damage done to surrounding objects.

Of course, it is readily understood that such an explosion cannot take place in a confined space without the rupture of the walls (the wire cannot be boarded over); but in every case that I have found recorded this dissipation takes place just as gunpowder burns when spread on a board. The objects against which the conductor rests may be stained, but they are not shattered. I would therefore make clear this distinction between the action of electrical energy when dissipated on the surface of a large conductor and when dissipated on the surface of a comparatively small or easily dissipated conductor. When dissipated on the surface of a large conductor,—a conductor so strong as to resist the explosive effect,—damage results to objects around. When dissipated on the surface of a small conductor, the conductor goes, but the other objects around are saved.

A Typical Case of the Action of a Small Conductor.

Franklin, in a letter to Collinson read before the London Royal Society, Dec. 18, 1755, describing the partial destruction by lightning of a church-tower at Newbury, Mass., wrote, “Near the bell was fixed an iron hammer to strike the hours; and from the fall of the hammer a wire went down through a small gimlet-hole in the floor that the bell stood upon, and through a second floor in like manner; then horizontally under and near the plastered ceiling of that second floor, till it came near a plastered wall; then down by the side of that wall to a clock, which stood about twenty feet below the bell. The wire was not bigger than a common knitting needle. The spire was split all to pieces by the lightning, and the parts flung in all directions over the square in which the church stood, so that nothing remained above the bell. The lightning passed between the hammer and the clock in the above-mentioned wire, without hurting either of the floors, or having any effect upon them (except making the gimlet-holes, through which the wire passed, a little bigger), and without hurting the plastered wall, or any part of the building, so far as the aforesaid wire and the pendulum-wire of the clock extended; which latter wire was about the thickness of a goose-quill. From the end of the pendulum, down quite to the ground, the building was exceedingly rent and damaged. . . . No part of the aforementioned long, small wire, between the clock and the hammer, could be found, except about two inches that hung to the tail of the hammer, and about as much that was fastened to the clock; the rest being exploded, and its particles dissipated in smoke and air, as gunpowder is by common fire, and had only left a black smutty track on the plastering, three or four inches broad, darkest in the middle, and fainter towards the edges, all along the ceiling, under which it passed, and down the wall.”

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SCIENCE

NEW YORK, JULY 28, 1893.

SYSTEMATIZED GRADUATE INSTRUCTION IN PSYCHOLOGY.

BY E. W. SCRIPTURE, NEW HAVEN, CONN.

Instruction in psychology cannot be said to have been placed on a sound basis till it consists of a series of carefully graded teaching from elementary text-book instruction to the highest kind of original work. Haphazard work here is just as bad as anywhere. It is self-evident that the student of psychology should properly apportion the amount of time spent in its various departments and in the other sciences he will have need of. The man who starts with the supposition that the way to study psychology is to go into the anatomical laboratory on the one hand and to take heavy courses in Greek philosophy on the other, is losing much valuable time. It is hereby not implied that no time is to be given to these subjects any more than that geometry and history are to be omitted from a man's education. But when a man has finished his college work and goes to the university he is supposed to have received his general culture and to be ready for his life-work.

The specialist is a man of broader knowledge than the dilettante. The difference between the two is that the latter browses at random, while the former reaches over a much wider field, but with a careful selection and coordination of the portions related to some central point. There is a maximum of energy and health which a man can employ in work; if this capital is invested in a careless way it will bring in small returns; the man will never really gain a complete training in anything.

The problem of a specialist is to go over as much ground as possible; to do this it is necessary to pass rapidly over the less valuable portions in order to have time for the valuable ones further on. Moreover, no essentials should be overlooked, no matter how distant they apparently lie. This last requirement is probably the most important of all. There is many a psychologist to-day who is fatally weak in some one or more points; it would be easy to find those who, although making measurements, know nothing of the science of measurement, or who, using light, heat, etc., as tools in their experiments, have little idea of the laws of the forces they are handling. To remedy all these defects in the dilettante way a man would have to study a couple dozen sciences; since life is too short to learn even one with any respectable thoroughness, the only way to do is to take just what will be of the most advantage to the psychologist, always bearing in mind that an hour too much on any one point means an hour too little on some other one.

It is the first problem of the psychological laboratory or the psychological department to so arrange its courses as to satisfy these requirements. As my own experience may possibly be of use to some one I will indicate briefly the outline of a system of instruction designed to meet this want. It is to be borne in mind that I am not speaking of college work with the object of general culture, but of serious university work for one who desires to study psychology.

As the science of psychology to-day is based on measurement and experiment, the work of the student must begin with some considerations on the method of making experiments; this should be followed by careful work in the theory of measurements, treating of the probability integral, the mean variation, etc. This work resembles somewhat the corresponding work given in physical measurements, but although the mathematical princi-

ples are the same, the treatment differs considerably. One of the great differences between psychological and physical measurements is that the conditions cannot yet be as accurately controlled as in physics; our mean variations are thus greater and the deductions we can draw from the results are not the same. In this respect psychological measurements on a single person somewhat resemble measurements taken once on each of a large number of persons. Partly for this reason, but mainly also for the sake of mental statistics, a study of the methods of statistics has to be made. The making of measurements brings in the study of fundamental and derived units and the construction of apparatus. The study of the various subjects of touch, sight, hearing, etc., requires a consideration of the physical processes used in stimulation. Thereafter the usual psychological subjects are, in a lecture course, to be treated in detail.

Hearing lectures will never make a psychologist; the fundamental course for all special instruction is the laboratory work. The student must be trained by repeated exercises in making the measurements explained in the lectures, including exercises on touch, temperature, hearing, sight, in the graphic method, chronometry, dynamometry, audiometry, photometry, colorimetry (psychological), etc. This should be followed by work in the construction of apparatus, elements of mechanical drawing, use of tools, etc. It is of great importance not to have too many men at work at the same time, at least not until psychological laboratories are much enlarged. During the past year the average attendance on this course in the Yale laboratory has been eight, an unpracticable number. Even with the enlarged equipment for the coming academic year, the number admitted to this practice course will have to be limited.

The object of university instruction, as distinguished from college training, is to develop the love of research, to train the student in research methods, to furnish him with the requisite knowledge and skill, and finally to provide him with the apparatus and other means of work for carrying out such investigations as may be best for him to undertake. The requisite knowledge of the psychological methods is gained from the laboratory course, the training in the difficulties and methods involved in research is obtained by placing the newer students as helpers to the advanced ones. The importance of this last arrangement can hardly be overestimated. It is the one in vogue at Leipzig and elsewhere.

It is a very dangerous thing for a man to take up a problem for investigation unless by previous experience with some one else he has found out that research is the hardest kind of work and has learned the thinking, the untiring patience, the courage under defeat that are called for at the various stages of work.

If we regard the research work as a means of training, it is an important matter to the student that he shall not undertake problems with rather indefinite boundaries or those where he may perchance run wild or be led into careless work. There can be no better training than that found in the investigation of a single point where the most careful measurements and manipulation are required. Once the student has learned the proper habits he will do far better work with suggestive and uncertain problems than could otherwise be hoped for.

If a student has had the proper general culture in philosophy, physics and mathematics, such a course as that outlined ought to make a thorough psychologist out of him. If he has not had the proper college training it behooves him to make it up as fast as possible. In the first place, an acquaintance with German is absolutely indispensable. Some acquaintance with the epistemological theories of the day is also necessary. A thorough scientist in psychology could not get along without knowing some-

thing about calculus, at least enough to follow the developments in such works as Müller's *Grundlegung* or Weinstein's *Physikalische Maassbestimmungen*. The more physics he knows the better.

OUR CRIPPLED WEATHER SERVICE.

BY JAMES P. HALL, BROOKLYN, N.Y.

A recent order of the new Secretary of Agriculture stops all the scientific research which, until this month, was being conducted by the United States Weather Bureau, and limits the functions of the experts in the Central Office to mere forecasting. Quite apart from all personal and political considerations, this is a lamentable event on many accounts.

It appears to be necessary, even in this enlightened age, to prove afresh that "pure science" is a prerequisite to most of our material progress. We are still under the necessity of making out that Columbus, who conceived that other lands lay to the westward of the great Atlantic, who visited one potentate after another to secure aid for his schemes, who haunted the courts and camps of Ferdinand and Isabella year after year, and who backed up his case with only the calculations of "pure science," really served Spain in particular, and civilization in general, quite as well as the "practical" men who handled the ropes and sails of the three caravels. We must elaborately demonstrate, all over again, to some of our fellow-countrymen that the unknown inventor of the mariner's compass and those other "pure scientists" who make charts showing the deviation of the needle, have conferred as great benefits on mankind as the pilot who uses that quivering bit of steel in bringing his ship safely across the seas. We must be prepared to face a question whether the captain of a New England fishing smack who thumbs his almanac to find out at what hour the tide rises or falls on a given day is not, after all, the superior (as an agent in civilization) to those learned astronomers and mathematicians who compute the tables for that little pamphlet. We must not be surprised if sane, intelligent, even eminent men, tell us that all the amazing development in steel production which we have witnessed in Europe and America in the last quarter of a century would have come just as soon—perhaps sooner—if Henry Bessemer had not carefully evolved his wonderful process from chemical theories and laboratory tests, nor ought it to startle us if some one insists that the sweating laborer in a rail mill, who grasps with tongs the fiery snake which emerges from the rollers and drags it away to have its ends sawed off, does more toward the building of a safe and lasting road than the expert who sits at a table and figures out the precise cross-section of rail that will give the greatest resistance to all the complex strains to which those bars must be subjected in service, even though these calculations extend over years and are based on long-extended and carefully designed tests. We cannot count on the universal acceptance of our opinion—if it happens to be our opinion—that Roebeling, in computing the exact size and number of the wires to hold up a bridge over East River, and in drafting all the plans for that wonderful structure, was at all comparable in usefulness with the truckman who now drives a two-horse team across it every day. If we positively assert that the projectors of the great railway systems beyond the Mississippi have done more than the men who drove spikes with sledge-hammers to open up that region to settlement and to provide outlets for the enormous grain and pork product which has resulted, we know not how soon nor how flatly we shall be contradicted. We may meekly hint that the physician who prescribes does as much to cure us as the drug clerk who compounds the prescription; that the arithmetic maker is as much of a public benefactor as the corner groceryman who foots up the total cost of ten pounds of sugar and two pounds of coffee; that Edison, who perfected the incandescent lamp after long years of experiment with no end of substances for his filament, did as much to give us an electric light as the man who tacks up cloth-covered wire in our offices and screws pear-shaped globes into wall-fixtures; that Graham Bell was quite as instrumental in enabling us to converse over a wire with people a dozen miles away as the patient girl who answers our ring and sticks a little brass plug in a hole for us; and that we owe as much to the long array of de-

signers, from Watts to Buchanan, who have brought the locomotive engine up to its present perfection, as the engineer on the "limited" express for the marvellous speed we make in going to Chicago; but we must not mistake for conviction the tolerance with which these utterances are received.

And so in meteorology. There are minds so constituted that they regard the observer as the equal or superior of the inventor of the barometer and thermometer; the "practical" man who jots down figures on a map and then draws "isobars," "isotherms" and wind signs on it as more useful than the pure scientist who, without touching pencil to paper, studies the movements of high and low pressure areas across the country, and the man who guesses what changes will occur during the next twenty-four hours, in the shape, size, position, intensity and other features of the cyclonic and anti-cyclonic systems, are doing better work than one who discovers and formulates the laws that govern those changes, and thus renders forecasting possible. What makes this the more amazing is the insufficiency of our present rules for weather predictions. The principles involved are not yet fully established. The most successful experts in this line realize that they are working under only a provisional code that must be greatly modified and supplemented. There is not a science so young and underdeveloped as meteorology; there is not a bureau in the national government whose maxims and procedure are not better established, nor, when one considers the immense and varied interests—railway, shipping, agricultural, commercial and individual—which are affected by the weather, is there any branch of the service which affects so many people, and affects them so directly, as this, unless we except the postal business? Not to strain every nerve to improve the quality and character of the work by fuller inquiry into fundamental theories is folly, if not crime. Such a policy of neglect involves direct waste, as ignorance always does. Our expenditure, year after year, would not thus be made to the best possible advantage. On the other hand, to use one per cent (\$10,000), out of the \$1,000,000 appropriated for the bureau, in expert work, would be a measure of true economy by gradually revealing how best to use the rest. That has been true of the bureau from the start; and it has never been a wiser course than it would be now. Any manager of a creamery, sawmill, cotton factory, iron foundry or railroad who deliberately threw away such a chance as this for improving what everyone recognized as the inadequate facilities of his business, at a trifling cost, would be set down by "practical" men as strangely blind or culpably reckless.

ANALOGOUS VARIATIONS IN SPHAGNACEÆ (PEAT-MOSSES).

BY H. N. DIXON, F.L.S., NORTHAMPTON, ENGLAND.

In the "Origin of Species" (6th ed., p. 126) there is the following passage, under the heading of "Analogous Variations": "As all the species of the same genus are supposed to be descended from a common progenitor, it ought to be expected that they would occasionally vary in an analogous manner, so that the varieties of two or more species would resemble each other, or that a variety of one species would resemble in certain characters another and distinct species,—this other species being, according to our view, only a well-marked and permanent variety."

A clear example of this is of considerable value in the support it gives to the theory of descent; but, as Darwin goes on to show, there are several reasons why such examples are not common.

A very striking illustration is, however, to be seen among the peat-mosses, or species of *Sphagnum*, and, as I do not know that anyone has drawn attention to the facts from this point of view, I think it may be of interest to present them briefly. Many of the facts quoted below are taken from a paper by C. Jensen (translated in the *Revue Bryologique*, 1887, p. 33, by F. Gravet), entitled "Les Variations Analogues dans les Sphagnacées."

Sphagnum acutifolium may be taken as a typical species of the genus; in its most characteristic form it is a plant with tall, slender stems, bearing at intervals fascicles of simple branches of two kinds, the one (divergent) stouter and more or less horizontal,

the other (pendent) longer, thinner, straight, and appressed closely downwards to the stem; the leaves on the branches being closely imbricated all round. The stem bears leaves very different in form and structure from those of the branches.

Now *Sphagnum acutifolium* is a most variable moss; the list of recognized species in Europe alone numbering about thirty.

Among these are several distinct and well-marked forms, such as the following: In one the branch leaves, instead of being straight and closely imbricated as described above, are bent back in the middle and spread almost at right-angles from the branch — the *forma squarrosa*. In a second the branches, instead of being straight or nearly so, are hooked or contorted — the *falcate* variety. In a third, the *forma compacta*, the whole plant takes a short, compact habit, the stems being much shortened and closely tufted, the fascicles of branches close together, and the branches themselves short and stunted, with the leaves closely set. In a fourth the differentiation between the stem and branch leaves almost or quite disappears, the former acquiring the form and structure of the latter, the *forma homophylla*, and so on with two or three more distinct varieties.

Now, if we turn to the other species of the genus, we find that of those found in Europe and North America there is hardly one which does not include one or more of these six or seven distinct varieties which we find in *S. acutifolium*. Thus of nineteen European species (all but two of which are natives of North America) sixteen, and perhaps eighteen, have varieties belonging to the *forma compacta*, fourteen at least, and perhaps four others, have the *squarrose* variety, and so on to a greater or less degree with the other forms. At least two of these forms are found under every one of the species, and in more than one species all the forms are found.

Here we have a clear case of analogous variations. It cannot be supposed that they are instances of reversion to a common ancestral form, for, apart from other considerations, the variation in some of the forms is in a directly opposite direction to that which it takes in others. The delicate, elongated forms of the *tenella* and the dense, compact forms of the *compacta* can hardly both be reversions to a common ancestral type!

So far we have exactly the same thing that we see in many races of domesticated species, such as Darwin has pointed out, for instance, in the races of the domestic pigeon; but we do not often see it carried out on such a wide and instructive scale.

But what is of especial interest in the case of the Sphagnaceæ is that, when we go further and consider the characters that distinguish the different species from one another, we find that the very points which we have seen mark off the above varieties (and render them, as a rule, more distinct than the other varieties of the species) are in several cases those which are most characteristic in separating from one another the species themselves. Thus *S. squarrosum* is especially marked by the spreading leaves; *S. rigidum* has for its most obvious features the very characteristics by which the *compacta* forms above described are distinguished; *S. subsecundum* in most of its forms is marked by its falcate or contorted branches; while a group of species, classed by Lindberg as *HOMOPHYLLA*, are characterized by that similarity of stem and branch leaves which I have described above as the feature of the corresponding variety; and so on with the other forms. Here we have exactly fulfilled the supposition of Darwin quoted above, "that a variety of one species would resemble in certain characters another and a distinct species," and fulfilled, too, on a scale which, at any rate, precludes the possibility of its being due to fortuitous coincidence.

On any theory of creation that did not presuppose a common ancestry for these species of Sphagnum, it might indeed be possible to account for the analogy between the varieties of different species by assuming the variations to be the direct results of the environment (a more than doubtful assumption, moreover); but the more we lay this cause under contribution to account for the varietal forms, the harder it is to believe that precisely the same variations in the species, only carried out to a higher degree of permanency, are due to entirely different and quite unconnected causes.

The above facts appear to me to form a peculiarly interesting

support to Darwin's argument from analogous variation. In the first place, the possibility of reversion is, as I have pointed out, eliminated, and reversion and analogous variation, which are quite distinct principles, are too often indistinguishable in their results for us to be quite certain that we have a genuine example of the latter. In the next place, as Darwin points out, analogous variations are liable to be eliminated as not being necessarily serviceable; that they are not eliminated in the Sphagna is, I believe, partly due to the peculiar conditions under which these plants usually grow, but this opens too wide a field to enter upon here. In addition to these reasons, we have here an illustration drawn from species and varieties in a state of nature; examples of analogous variations have usually to be drawn from domesticated forms, where their value is limited by their necessarily applying to races and varieties only, and not to distinct species.

I append a table (taken from Jensen's paper quoted above), which shows at a glance the distribution of these varietal forms among the European species of Sphagnum. A † indicates the existence of the variety heading the column under the species opposite to which it is placed; a ? means that the existence of such a form is probable, but is insufficiently attested or not clearly enough marked to be entered as certain. It must be remembered that there is always a possibility of gaps being filled up by future research, but the table is, I think, as it stands, sufficiently striking.

Group.	Species.	Forma homophylla.	Forma compacta.	Forma tenella.	Forma falcata.	Forma squarrosum.	Forma imbricata.
Sphagna cuspidata.	<i>Sphagnum laxifolium</i> , C. M.....	†			†	†	†
	" <i>intermedium</i> , Hoffm.....		†	†		†	†
	" <i>riparium</i> , Angstr.....					†	†
	" <i>lindbergii</i> , Schimp.....		†			†	†
	" <i>wulfii</i> , Girg.....		†			†	†
	" <i>acutifolium</i> , Ehrb.....	†	†	†	†	†	†
	" <i>strictum</i> , Lindb.....		†			†	†
	" <i>fimbriatum</i> , Wils.....		†			†	†
	" <i>teres</i> , Angstr.....		†			†	†
	" <i>squarrosum</i> , Pers.....		†			†	†
S. subsecundum.	" <i>subsecundum</i> , Nees.....	†	†	†	†	†	†
	" <i>caricium</i> , Spruce.....	†	†	†	†	†	†
	" <i>tenellum</i> , Ehrb.....	†	†		†		†
S. compacta.	" <i>compactum</i> , D. C.....		†			†	†
	" <i>molle</i> , Sull.....		†			†	
	" <i>angströmii</i> , C. Hartm.....		†			†	†
S. palustre.	" <i>cymbifolium</i> , Ehrb.....	†	†			†	†
	" <i>papillosum</i> , Lindb.....		†			†	
	" <i>austriale</i> , Sull.....		†			†	†

THE CLOSE OF THE ICE AGE IN NORTH AMERICA.

BY R. W. MCFARLAND, LL.D., LATE PRESIDENT OF MIAMI UNIVERSITY.

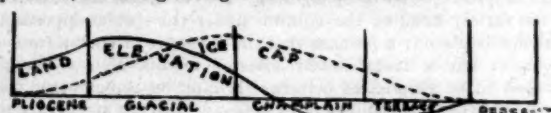
THIS is a question of interest to scientific men in general, and to geologists and glacialists in particular.

In Professor Wright's "Ice Age in North America," p. 448, in speaking of Croll's table of the eccentricity of the earth's orbit, he says: "According to this table the modern period most favorable to the production of a glacial epoch began about 240,000 years ago, and ended 70,000 years ago." Again, on p. 450, we have this: "If, therefore, the glacial period should prove to have ended only 10,000 years ago, instead of 70,000, the Darwinian would be relieved of no small embarrassment."

A genuine scientist, of course, has no preconceived theory to

sustain — whether of Darwin, or of Archbishop Usher — he seeks only to know the truth, whatever may be the consequences. Perhaps the points mentioned in this paper further along have not had sufficient attention hitherto. "Come, let us reason together."

The first extract above sets the close of the "Ice Age" entirely too far back. One of the objects of this paper is to make good this assertion. From the facts set forth below, it is reasonable to conclude that even on Croll's theory alone the close was not over 40,000 years ago, and possibly not over 35,000. If the views of Professor LeConte, given in his paper of January, 1891, have the weight and influence which their importance demands, it seems to me that there need no longer be any contest between glacialists who reject the astronomical theory, by reason of the remoteness of the time, and those who refer the ice age to terrestrial causes alone. Professor LeConte's theory is so clearly and tersely set forth that it is best to quote it entire, as given by Professor Wright on pp. 618-9, including the figure used in illustration:—



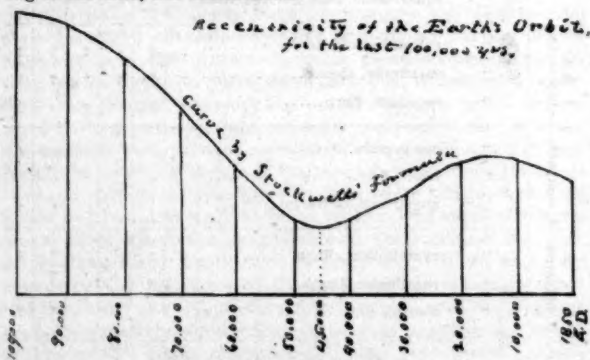
"1. That the continental elevation which commenced in the Pliocene culminated in the early Quaternary, and was, at least, one of the causes of the cold, and therefore of the ice accumulation.

"2. That the increasing load of ice was the main cause of the subsidence below the present level.

"3. That the removal of the ice-load by melting was the cause of the re-elevation to the present condition.

"4. That all these effects lagged far behind their causes.

"This lagging of effects behind their causes is seen in all cases where effects are cumulative. For example, the sun's heating power is greatest at midday, but the temperature of the earth and air is greatest two or three hours later; the summer solstice is in June, but the hottest month is July; and in some cases the lagging is much greater. The cause of sea-breezes, — i.e., the heating power of the sun, — culminates at midday, but the effects in producing air-currents culminate late in the afternoon and continue far into the night, long after the reverse cause, i.e., the more rapid cooling of the land, has commenced.



"Now, in the case under consideration, it is probable that the lagging would be enormous in consequence of the reluctant yielding of the crust and the capacity of ice to produce the conditions of its own accumulation. Although the elevation produced the cold, and therefore the ice accumulation, yet the latter culminated long after the former had ceased, and even after a contrary movement had commenced."

So far LeConte.

The close of the glacial epoch as above given — 70,000 years — is wholly arbitrary, and is evidently very far from the truth, as will be shown. At that time the eccentricity of the earth's orbit was nearly twice as great as it is now, and the consequent excess of the sun's time on one side of the equator above that on the other side (depending on the longitude of the perihelion) was

about fourteen days. It had decreased from thirty-five, when the difference was greatest. But this difference of fourteen days would work in the direction of great difference of climate between the hemispheres, and would so continue to work as long as there was any difference at all. And not only so, but the effect would continue and accumulate according to the universal law of nature in the cases above cited, long after the smallest eccentricity had been reached. And that smallest eccentricity occurred less than 45,000 years ago, whether the computation be made by the formula of LeVerrier or by that of Stockwell.

The last sentence of the extract from LeConte is significant: "Yet the latter culminated long after the former had ceased and even after a contrary movement had commenced." In this latitude the usual temperature for the first week in December is not very different from that of the first week in March. Yet the sun in the first case is more than twenty-two degrees south of the equator, and at the latter date is scarcely five degrees. In like manner, and in accordance with the law above named, suppose the intense cold resulting from the wide glaciation of the northern parts of this continent, to have continued long after the eccentricity had reached its minimum, then it is not only possible, but even probable, that the close of the ice age was not more than 35,000 years ago, even if 30,000 would not be a more accurate designation. In which event, the objection to the astronomical theory, by reason of the long time elapsed since the days of high eccentricity, is wholly removed. And Professor Wright himself, although long favoring the short period of 10,000 years, has lately seen cause to doubt whether this is not too small. In a letter to the *New York Nation*, under date of Sept. 15, 1892, in view of his recent investigation of the old northern outlet of the great lakes into the Ottawa River, he says the facts "will . . . considerably lengthen our computation."

This throwing back of the close of the ice age by the glacialist, and the preceding shortening of the period in accordance with a universal law of nature, may both serve to strengthen the hypothesis of LeConte, and commend it to all interested in these questions, as the explanation which best accounts for the admitted facts.

CURRENT NOTES ON CHEMISTRY.—I.

[Edited by Charles Platt, Ph.D., F.C.S.]

Properties of Diamonds.

THE experiments of M. Moissan in the production of artificial diamonds and other precious stones, his remarkable results in the reduction of the most refractory oxides and his whole line of work at high temperatures, are well known to readers of the scientific magazines. Some of the more recent investigations have been of the properties of the diamond when heated in oxygen, hydrogen, chlorine, etc. When the temperature is raised slowly the combustion is correspondingly slow and without production of light, being recognized solely by the action of the gas evolved on baryta solution. At 40°–50° above the point at which this slow combustion takes place the combustion becomes more rapid, producing a visible flame. Yellowish-brown carbonado burned with a flame at 600°; black carbonado with a flame at 710°–720°; transparent Brazilian diamond without a flame at 710°–730°; transparent crystallized Brazilian diamond without a flame at 760°–770°; cut diamond from the Cape without a flame at 780°–790°; Brazilian bort and Cape bort without a flame at 790°, and with a flame at 840°; very hard bort without a flame at 800°, and with a flame at 875°. As a rule, the harder the diamond the higher its point of ignition.

When heated to 1200° in hydrogen the Cape diamond loses nothing in weight, but becomes lighter in color and less transparent; dry chlorine and dry hydrogen fluoride have no action at 1100°–1200°. Sulphur attacks diamonds at 1000°, but with carbonado carbon bisulphide is readily produced at 900°. Sodium vapor has no action at 600°. Iron at its melting point attacks the diamond with the production of graphite on cooling; melted platinum also combines readily. Fused potassium hydrogen sulphate and alkali sulphates, potassium chlorate and nitrate are all without action on the diamond, but, according to Damour, attack

carbonado. The diamond is rapidly dissolved when heated to a high temperature with carbonates of the alkalis, carbonic acid being given off, but no hydrogen, and hence M. Moissan concludes that diamonds contain no hydrogen or hydrocarbons.

Treated with hydrofluoric acid, and then with aqua regia and finally washed, dried and burnt in oxygen, the diamonds yielded an ash consisting in all cases but one chiefly of ferric oxide. Cape bort contained also silica, calcium and magnesium, and Brazilian carbonado, silica and calcium, with a trace of magnesium. One specimen of green transparent bort from Brazil left a minute quantity of ash, which contained silica, but no iron.

Preparation of Pure Alumina.

The preparation of pure alumina from bauxite, which is always accompanied by more or less silica and oxide of iron, has commonly been carried out as follows: Taking advantage of that property of alumina, which enables it to act as either base or acid, according to its environment, the bauxite is fused with sodium carbonate, the resulting products being sodium aluminate and sodium silicate. The mass is then extracted with water and the sodium aluminate passed into solution. The silicate of soda, owing to a deficiency of base, is but little acted upon by the water and with the ferric oxide is left in the residue. From the solution of the sodium salt the alumina is precipitated by passing carbonic acid gas, carbonate of soda being regenerated at the same time.

This process has lately been improved by first precipitating a portion of the solution of aluminate by the gas in the cold, producing a small quantity of crystallized alumina hydrate of the same composition as Gibbsite, $Al_2O_3 \cdot 3H_2O$. This, then, is added to the main bulk of solution, and a complete separation of the whole is secured, the soda being left in the caustic state. The reaction which takes place has been investigated by M. A. Ditte, and is explained as follows: A solution of sodium aluminate may be regarded as amorphous hydrated alumina dissolved in caustic soda. The form in which a body crystallizes from a solution is largely determined by the character of the crystal introduced to start crystallization. Hence in the process described above the tendency of the whole is to crystallize in the form of the several crystals first introduced, and as the crystalline form of alumina is less soluble than the amorphous in alkaline solutions, there is a gradual complete precipitation. Stirring facilitates the separation of the crystals by bringing those already formed into contact with fresh portions of solution. There is finally left only that proportion of alumina corresponding to the solubility of Gibbsite in caustic soda under the conditions existing.

Silk from Wood.

At the Paris Exposition in 1889 M. de Chardonnet gave to the world his process for the manufacture of silk from wood and received the highest honors from the jury of award. Since that time the process has been further developed and has presumably attained a practical success; silk is being manufactured at Besançon from wood pulp such as is used in the fabrication of certain kinds of paper. According to F. B. Loomis, U. S. Consul at St. Etienne, the following process is used: The pulp is carefully dried in an oven and plunged into a mixture of sulphuric and nitric acids, then washed in several water-baths and dried by alcohol. The product thus prepared is dissolved in ether and alcohol with the production of collodion similar to that used in photography. This collodion, which is sticky and viscous, is enclosed in a solid receptacle furnished with a filter in the lower end. An air-pump supplies air to the receptacle, and by its pressure forces the collodion through the filter, removing all impurities. The collodion flows into a horizontal tube armed with three hundred cocks having glass spouts pierced by a small hole of the diameter of the thread of a cocoon as it is spun by the silkworm. The spinner opens the cock and the collodion issues in a thread of extreme delicacy. This thread, however, is not yet fit to be rolled upon spools on account of its viscosity and softness, being still collodion and not "silk." To obtain the necessary consistency the thread as it issues is passed through water, by which means the ether and alcohol are washed out and the collodion solidified

and transformed into an elastic thread as brilliant and resisting as ordinary silk. The dangerous inflammability of this substance, as prepared above (two centimetres per second), has been reduced, according to the inventor, by passing the spun thread through a solution of ammonia, thus rendering it as slow of combustion as any other like dress material.

Up to January of this year none of the more important silk manufacturers of St. Etienne or Lyons had invested heavily in this enterprise, but all express confidence in the process and believe it is destined to figure largely in the commercial world.

New Method for Melting Points.

A. Potilitzin is the author of a new method for the determination of the melting points for substances melting below 450° , this being the highest temperature which a nitrogen-filled mercury thermometer can indicate. One end of a hard-glass tube, 5 mm. bore and 500-600 mm. in length, is drawn out to capillary fineness and the other is bent at right angles. The capillary is dipped into the molten substance, the melting point of which is to be determined, so that on cooling the tube is closed by a solid plug of the substance 3-4 mm. long. The other end is connected with a manometer by means of which a pressure exceeding that of the atmosphere is maintained within the tube. The tube, along with the principal thermometer and also one for stem correction, is inserted into a wide test-tube, which is then immersed in a bath of fusible metal. When the melting-point is reached the plug softens and is expelled by the internal pressure, so that the sudden equalizing of the pressure in the manometer indicates the moment when the substance melts, the thermometers being then read off. Potassium nitrate was found by this method to melt at 336.57° (mean of eight experiments); by immersion of the thermometer direct into a large mass of the salt the melting point was found to be 336° .

Pigments Used in Some India-rubber Toys.

India-rubber has been generally, and correctly, accepted as a suitable material for children's toys; but investigation into the manufacture of the latter reveals the fact that many as placed upon the market contain harmful ingredients. A. Bulowsky has recently called attention to several dangerous ingredients as, for instance, in black dolls, which are often colored "in the mass" with lead pigments. Red articles are also most usually colored in mass, the pigment being antimony sulphide, which, however, being unattacked by the saliva may be considered innocuous. Grey rubber goods generally contain zinc oxide, and hence particularly when, as is sure to be the case, the toy is brought to the child's mouth, an element of danger is introduced. Superficial coloring is frequently accomplished by means of poisonous pigments. These remarks are applied in particular to foreign manufactures; and though, doubtless, the same coloring matters are used in this country, I have yet to learn of a case of poisoning from coloring in mass. Superficial pigments, from their disposition to flake and from the greater quantity brought into contact with the mouth, are certainly to be avoided. It is difficult, moreover, to estimate the amount of damage done by these toys owing to the many petty ills and derangements of infancy, the poison received by the child very likely is insufficient to develop well-defined symptoms or to direct suspicion, but at the same time may be the cause of an indisposition which itself brings on crying, wakefulness, and general wear on the little body struggling for existence.

NOTES AND NEWS.

PROFESSOR E. W. DORAN has been elected president of Buffalo Gap College, Buffalo Gap, Texas, and has resigned his position at College Park, Md.

— C. H. Turner has resigned his position at the University of Cincinnati and accepted the Chair of Natural History at Clark University, Atlanta, Ga.

— P. 21, line 9, from below: "registation," read "registration;" p. 22, line 15, from below: "possible," read "impossible;" and p. 22, column 2, line 31, from above: "understood," read "understand."

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Attention is called to the "Wants" column. It is invaluable to those who use it in soliciting information or seeking new positions. The name and address of applicants should be given in full, so that answers will go direct to them. The "Exchange" column is likewise open.

THREE SHIPS WITH BERIBERI OUTBREAKS SHOWN TO HAVE HAD EXTENSIVE FORMATION OF CARBONIC OXIDES DURING THE VOYAGE—ANALYSIS OF BERIBERI BLOOD—CONCLUSION THAT BERIBERI IS NOTHING BUT CARBONIC POISONING OF THE BLOOD.

BY ALBERT S. ASHMEAD, M.D., NEW YORK, N.Y.

In *Science*, Nov. 18, 1893, I contributed an account of the outbreak of beriberi on board the bark "H. B. Cann," from Ilo-Ilo, Philippine Islands, with a cargo of raw sugar. That cargo fermented during the trip, stifling fumes filled the ship, and the beriberi outbreak was considered the consequence of this state of things.

In an article which will shortly appear in the *Medical News*, entitled "Investigation of the Outbreak of Beriberi on Board the Bark 'Pax' from Ceylon with a Cargo of Graphite," I show that, from the deficient packing of 1,300 tons of graphite, the cargo was exposed to the moist air encountered on a tropical voyage, all but six of a crew of nineteen were stricken with beriberi.

The bark "J. C. Warns," from Java and Macassar, with a cargo of green coffee, arrived in New York June 23, 1890. The captain and three men had died of beriberi. The coffee had been picked and shipped too green. Mr. Tobias, consignee of the cargo, showed me a sample of it; it was charred, carbonized, and almost destroyed. The coffee had fermented. The outbreak of beriberi on a ship from Java, where the coffee has been carbonized, is a regular occurrence. Java coffee owes its value in our market to its color; in order to obtain this color, the captains take their cargoes quite green, which favors a slight fermentation during the trip. Sometimes they go too far; the coffee is too green, and the fermentation too violent; in such cases there is always carbonization; the grains stick together in great masses, and abundant fumes (carbonic gases) fill the ship.

The iron ship "Glenmorang," Captain Currie, 133 days from Colombo, Ceylon, with 1,100 tons of graphite on board, 800 tons of cocoa-nut oil, etc., arrived July 17 in New York. This ship, loaded in Colombo alongside the "Pax" (mentioned above), travelled the same course, at an interval of two weeks. She lies now at the Atlantic dock, in Brooklyn, again alongside the "Pax." She had no beriberi outbreak. From her first mate I have the following information:—

Crew, 28 men; captain's wife and two children on board; or all, 31.

She is a Scotch Glasgow boat, and the crew is English and Scotch. Before taking this cargo, this ship had carried from Barry Dock, near Cardiff, a cargo of coal to Buenos Ayres, South America, and taken a ballast of sand to Colombo, Ceylon. Before these trips she had been in the wheat trade from Tacoma, Washington, to Havre, France. She is remarkably dry, and the cleanest ship one would wish to see. I went down her hold and examined every part of it; there is not a smut nor a stain anywhere about it. The iron part is especially clean: no trace of incrustation of carburetted iron, which might have indicated the action of hot moist

air on the carbon. None of the barrels containing the graphite was broken. The packing was exceedingly good, the dunnage consisted of sticks and cocoa-nut hulls, so that it was impossible for the barrels to roll and break, and thus expose their contents to the action of the air.

The diet bill was about the same, or even poorer, than that of the "Pax." Nine casks of salted beef and seven barrels of pork were consumed during the trip. Fresh beef (tinned) three times a week, one-half pound to a man, and a half pound of salt meat on the same days; other days a full pound of salt meat a day. One-half pound of rice for each man on Saturdays; no vegetables except onions with the soup three times a week. The ship being Scotch, oatmeal made part of its fare for two and a half months after starting, when it ran out. No sickness whatever during the voyage. One death by accident. The captain attributes the good condition of his cargo and his crew to the change of winds and cooler weather which he enjoyed from the Cape of Good Hope to the North Atlantic. His log is indeed very different from that of the "Pax."

In *Science*, vol. xxii., No. 545, p. 16, Venable states that "the metallic carbides are usually formed by the action of intense heat upon the metal in the presence of carbon. The form of this carbon is capable of being greatly varied. Graphite, amorphous carbon, and many hydrocarbons, may be used. The heat of the ordinary furnace is sufficient to form the carbides of the metals already mentioned, zinc, copper, and notably iron. All of these carbides, under certain conditions, give off their carbon in the form of hydrocarbons. The same smell can be detected in all during their decomposition. In some cases, as iron and zinc, the decomposition is caused by the action of an acid. The carbides of the earths (of which graphite is one, in conjunction with iron) decompose in moist air, and more rapidly in water."

I may point again here to those broken barrels of the "Pax," which exposed the carbon to the influence of tropical air.

I have examined microscopically the blood of four of the sufferers of the "Pax," and obtained the following results: Captain Geesecke, sick since May 16 with beriberi; 900 diameters, $\frac{1}{8}$ inch oil-immersion objective; red discs, irregular in outline, congregated in masses, with ragged edges, not inclined to form rouleaux; quite plastic; colored streaks or rays of pink and red, showing the presence of biliary matter, biliverdin crystals; black spores, not free but entangled in the hummocks of corpuscles. It may be noted that the oedema of this patient's legs only left him two days before this examination.

Henry Oelrichs (second mate), German, 27 years old. Has been fourteen days sick with beriberi. Examination of the blood: 500 diameters, $\frac{1}{8}$ inch objective; red corpuscles, very plastic, aggregated in hummocks. Many black spores are seen floating about, free in motion. Fibrine in excess, light in texture, and lumpy. Blood very thick, syrupy, and plastic. No motion, showing want of circulation. Excess in the coloring matter. This same case examined: 900 diameters, $\frac{1}{8}$ inch objective immersion lens, shows excess of fibrine in ropes, biliary matter in great excess; no crystalline formations; blood quickly oxidizes and forms a solid mass. The black spores above mentioned are quickly held by the fibrine; the red discs are distended, bladder-shaped, and have very ragged edges. The meniscus-shape is lacking, there being great irregularity in outline and color, some are even square-shaped. Some discs have an excess of color, some are very pale. On the edges of the corpuscular mass the color quickly disappears, in consequence of rapid coagulation.

Isaac Heggund, a Swede, 27 years old, has had beriberi since crossing the equator, six weeks ago. Legs are now very thin, but still some soreness remain; knee reflexes still lost. First sound of heart prolonged. Microscopical examination of the blood, 900 diameters, $\frac{1}{8}$ inch objective, shows rouleaux well formed, no spores, no filaments, slight feverish condition shown by spiculated outlines of some of the red corpuscles. Fibrine is assuming a normal form, showing meshes very regular; no distension of red corpuscles.

Emil Jensen, a German, 19 years old, sixteen days sick with beriberi. Black spores in active motion and very plentiful; freely scattered in the field of observation; circulation very torpid; fibrine

very irregular, light in texture; biliary matter freely scattered; blood discs distended and with ragged edges; red corpuscles congregated in masses; fibrine forming in heavy clots; blood rapidly coagulating; black spores are quickly fastening in the fibrine.

We have here, in the 14th day sick and the 16th day sick cases, black spores in active motion and biliary matter in both cases, and the corpuscles distended bladder-shaped, in ragged-edged condition; the fibrine quickly clotting. And in the captain's case, which was the worst of all, we have still black spores, biliary matter, and ragged-edged corpuscles.

In the 6th week case, a much milder case, moreover, than any of the others, it is reasonable to assume that in some way the patient has quickly eliminated the poison. There is no biliary matter in his blood, no black spores, no abnormal fibrine, no distension of red corpuscles; the latter are perfectly formed in rouleaux.

Examination of urine, of Henry Oelrichs (second mate, bark "Pax"), July 17th, 1893 (14th day of beriberi):—

Odor, light, aromatic, and feverish.

Color, light (yellow) amber.

Reaction, excessively acid.

Appearance, transparent.

Specific gravity, 1.032 +.

Weight of a fluid ounce, 470.27 grains.

Solids in a " " 35.06 "

Nature of deposit, mucus.

Quantity of deposit, trace.

Bile, coloring matter not present.

Salts, " " "

Sugar, Fehling's solution, trace.

Chromate solution, "

Nylander's solution, "

Saccharimeter grammes in a litre, 0.00 +.

Albumen, nitric acid, 1 fl. $\frac{3}{4}$, not present.

Picric solution, trace.

Touret's solution, "

Bichromate solution, not present.

Bichloride solution, trace.

Millard's solution, "

Polariscopic grammes in a litre, 0.00 +.

Microscopical appearances:—

Pus corpuscles, trace in quantity.

Epithelium, bladder, trace in quantity.

Quantitative examination:—

Urea, proportion per fluid ounce.....	6.605 grains.
Percentage of.....	1.404
Total, quantitative examination....	66.050 grains.

Chlorine.....	.960 grains.
	204
	9.600 grains.

Sulphuric acid.....	.992 grains.
	210
	9.920 grains.

Phosphoric acid.....	1.024 grains.
	201
	10.240 grains.

Carbonic acid gas.....	1.120 grains.
	287
	11.200 grains.

Results on a net basis:—

Urea.....	1.40
Water.....	95.00
Sugar.....	0.00 +
Foreign.....	2.76
Albumen.....	0.00 +
Chlorine.....	.20 +
Sulphuric acid.....	.21
Phosphoric acid.....	.20
Carbonic acid gas.....	.23
	100.00

Traces of sugar and carbonic acid gas are commonly observed in the urine of beriberi patients.

Dr. Wallace Taylor, Osaka, Japan, sends me three interesting tables, which he made from examinations of 184 cases of beriberi. These examinations were made with Hayem's hematometer and Gower's hæmacytometer. The average corpuscular richness for the 184 cases is 94 per cent. This, he says, corresponds to the clinical experience in cases of beriberi. Most of the cases of beriberi seen by the general practitioners are well-fed, well-nourished, full-blooded appearing men. The ill-fed, poorly-nourished, weak constitution cases are the exception. During the past few years he has kept a record of the physical condition of the beriberi patients, and he gives this record, together with another record, of a beriberi hospital in Tokio:—

	Taylor.	Beriberi Hospital.	Sum.
Of strong constitution,	323	598	916
" average "	15	27	42
" weak "	9	6	15

Thus, in a total of 973 beriberi patients, there were 94 per cent of strong constitution (a result almost identical with that given in his tables), and only 6 per cent of average and weak constitutions.

"These numbers," says Taylor, "are large enough to be conclusive, and anemia is not one of the pathological conditions of beriberi."

In his table No. 3 there is shown a general diminution of the hæmoglobine. The average hæmoglobine in 101 cases is 81 per cent. In some of these cases the amount is very low, being below 65 per cent, and with but few exceptions the per cent of hæmoglobine is below the per cent of corpuscles, showing a deficiency of the individual corpuscles in hæmoglobine.

The appearance of biliary matters, which I have shown in my analyses of the four cases of the bark "Pax," would show by itself a deficiency of hæmoglobine.

In the *Tribune Médicale*, Sept. 10, 1891, Messrs. Bertin-Sans and Moitessier show that it is the presence of hydrogen and carbonic acid in oxycarbonized blood that prevents the total destruction of hæmoglobine.

By sweeping their solution of oxycarbonized blood and water, with a current of hydrogen and carbonic acid gas, and an addition of sulphide of ammonia, they obtained the spectrum of reduced hæmoglobine. They thus show that oxycarbonized hæmoglobine can be readily transformed into a mixture of methæmoglobine and oxide of carbon. It is therefore reasonable to suppose that in an outbreak of beriberi where we have the presence of oxides of carbon and a deficiency of hæmoglobine (observable in all cases of beriberi) the latter is the effect of the former.

In Japan, the universal burning of charcoal produces the oxides, which held down in the low places by the moist atmosphere of the beriberi season, there is produced on a large scale and continually during the moist season what happens on board of each of those ships which come to us from the East with carbonized cargoes and beriberi-sick crews.

THE STRUCTURE AND AFFINITY OF THE PUERCO UN- GULATES.

BY CHARLES EARLE, B. SC. (PRINCETON).

The discovery in 1880 by Baldwin of the presence of mammalian remains in the Puerco beds of New Mexico, was one of the most important in the history of vertebrate paleontology in this country. This rich mammalian fauna has been wholly described by that able investigator, Professor E. D. Cope, and to him we are indebted for having made known to the scientific world the interesting mammals which are imbedded in this formation.

As I have lately been studying a collection in the American Museum of Natural History from the Puerco, I propose in this paper to sum up some of the results of my investigations as relating in particular to the primitive ungulates of this formation, and especially to attempt to place some of these forms in or near their proper phylogenetic positions in the system.

As a word of introduction I would remark that most of the remains from the Puerco are in a poor state of preservation, and this applies particularly to the skeleton. The teeth are often well preserved, so that in working out the affinities of these mammals we are generally dependant upon the character of the teeth to discover their relationship to other forms. A very striking peculiarity in the dentition of the Puerco mammals, as pointed out by Professor Cope, is the fact that their superior molars are generally of the tritubercular pattern, and these upper teeth are associated with inferior molars, which are tubercular-sectorial, or a modification of the latter. In the tubercular-sectorial tooth the anterior portion is raised above the posterior or talon, and consists of three elevated cusps. By the modification of the latter pattern of molar, both the highly specialized sectorial teeth of the Carnivora and the quadritubercular teeth of the Ungulates have been derived.

In general we may say that, besides the characters of the teeth, the mammals of the Puerco epoch, in their skeletal structures, as far as known, are of a decidedly primitive type. The skull is short and heavy, with the orbit well forward over the teeth; the various processes of the skull for muscular attachment are prominent. Correlated with their low structure in general was the exceedingly small brain, as illustrated by the genus *Periptychus*. As the structure of the skeleton in the latter genus is the best known, I will enumerate some of its characters. The feet of *Periptychus* were plantigrade. The hind foot had five toes, the external one being not much shortened. The structure of the astragalus is well known in *Periptychus* and important, as teaching us one of the characters of the structure of the primitive foot in general. This bone is short and strongly depressed; the neck of the same is short and heavy. In all modern mammals which are digitigrade the trochlear surface of the astragalus, articulating with the tibia, is deeply grooved, whereas in *Periptychus* this surface is plane and flat. Another very important and primitive character of the astragalus in *Periptychus* is that it is perforated by a well-marked foramen. I am not aware that this perforation of the astragalus occurs in any recent Ungulate. The astragular foramen is of constant occurrence in Puerco mammals and also is present in some of their descendants in the Wasatch (*Coryphodon*).

In one respect the foot of *Periptychus* is more advanced than that of the genus *Phenacodus*, which is from a later formation (Wasatch); I refer to the articulation of the cuboid bone with the astragalus, but in general the foot structure of *Phenacodus* is far advanced in its evolution over that of *Periptychus*. *Phenacodus* was a digitigrade mammal, with the outer toes much shorter than the mediap. The long bones of the skeleton in *Periptychus* are short and heavy; this applies particularly to the humerus, which is an exceedingly heavy bone; its distal extremity is perforated by an entepicondylar foramen, another primitive character of this genus. The character of the humeral condyles in *Periptychus* is peculiar and different from all modern Ungulates. In the latter group the trochlear surface of the humerus is interrupted by a strong ridge separating the external from the internal articular surface. Now in *Periptychus*, as well as in *Phenacodus*, there is no such interruption of the condylar surface of the humerus, and it has the same character as in the modern Carnivora, thus showing how these two widely separated orders at the present time approach each other in their osteological characters in the Eocene. The ulna and fibula are large in *Periptychus* and more nearly approach the size of the bones of the preaxial side of the limbs than in modern forms.

Now the question arises, what great groups of mammals of later epochs than the Puerco are represented in this formation. I think that we may safely say that there were only a few main stems of Puerco mammals which persisted until later periods, and I shall endeavor to show that these stem forms were the direct ancestors of later types. As in so many cases, in seeking to determine phylogenetic relationships, we must turn to the investigations of Professor Cope to decide this question in part at least. He has described mammals from the Puerco which he considers to be Ungulates in their affinity, others to be related to the Carnivora, and still other types which resemble the Lemu-

roides in the structure of their teeth. As I am only dealing with the Ungulates in this paper I shall speak of certain genera which Professor Cope and other paleontologists have determined to be closely related to this group.

The group of primitive Ungulates which Professor Cope has designated the Condylarthra is not a very homogeneous one, it appears to me, and perhaps with Schlosser we had better speak of a condylarthrous stage, through which all Ungulates are supposed to have passed rather than to attempt to confine these early forms all in the suborder Condylarthra. At least as shown by Professor Osborn, the characters laid down by Professor Cope as limiting the Condylarthra, would not include some of the forms (*Periptychus*) which Professor Cope has embraced in this suborder.

When we attempt to separate the Ungulates from the Unguiculates of the Puerco we are met with the obstacle that in most cases the distal phalanges of the feet have not been preserved. Accordingly we are dependant upon the character of the dentition to diagnose and separate these two groups. However, so low down geologically speaking as the Puerco, the different groups of Ungulates are not supposed to be distinctly differentiated, and then again in most cases the structure of the skeleton, and especially of the carpus and tarsus of these forms, is totally unknown. I believe, however, that the stems leading to the main types of the Ungulates which we meet with in the Wasatch, are fairly well separated in the Puerco, and more so than has been generally accepted. For example, when we consider another group other than the Ungulata, the Creodonta, we find a number of well-marked families of this order in the Puerco, which are distinct and lead up in some cases to types of later epochs. The Creodonta, with low-crowned, purely bunodont teeth, such as are included in the Triisodontidae, the more specialized and trenchant dentition of the Provivirridae (*Deltatherium*), and again the low-crowned and nearly quadritubercular lower molars of the Arctocyonidae (*Claenodon*, Scott). The last-named genus is very likely the ancestor of the Wasatch (*Anacodon*).

Turning again to the Ungulates, what are the types of this order which we can distinguish in the Puerco? To attempt to decide this question we must rely on the characters of the teeth in nearly all cases. To ascend to the mammals of the Wasatch period for a moment we observe in that formation the Perissodactyles are distinct from the Artiodactyles. The former group has superior molars with six cusps, which may be either distinct or fused; the lower molars are quadritubercular. In the Artiodactyles of the Wasatch the superior molars are of the tritubercular pattern and the lower teeth are sextitubercular, or more nearly of the primitive tubercular-sectorial type already mentioned. Again, the premolars of the Perissodactyles are more complex than these of the Artiodactyles. Returning to the Puerco we find the same state of things well foreshadowed, although these two stems may have not passed the condylarthrous stage. In the genus *Euprotogonia* (= *Protoponia*), we have the supposed condylarthrous representative of the Perissodactyles, and in the genus *Protopogonodon* of the Puerco I believe we are dealing with an ancestral Artiodactyle. I am aware of the fact that the skeletons of these two genera are totally unknown, so until they are discovered we will be unable to say whether these two forms were true Condylarthra or if they had assumed more of the characters which are typical of the two great divisions of the Diplarthra. I think that from a study of the teeth of the above genera that the two lines of the Diplarthra were fairly well separated even in the Puerco.

The upper true molars of *Euprotogonia* in the typical form, *E. puericensis*, consist of six tubercles. The superior premolars are simpler than in *Phenacodus*. A character of the upper molars of *Euprotogonia*, and separating it well from *Phenacodus*, is the absence of the parastyle and mesostyle. When we study the structure of the lower teeth in *Euprotogonia*, we are surprised to find them so highly developed for a Puerco form. The last lower premolar is nearly as complex as it is in the Wasatch *Phenacodus*, and in the typical species the crescents of the inferior true molars are as plainly marked as in the last-named genus. In

the supposed ancestors of the Artiodactyles from the Puerco (*Protagonodon* and perhaps other genera, as suggested by Professor Scott) the characters of the dentition are well differentiated from those leading to the Perissodactyles. I have referred upper teeth in the American Museum collection to *Protagonodon*, which are of the tritubercular type, with exceedingly brachydont crowns. These upper teeth differ considerably from those of the bunodont *Creodonta*. The internal cones and intermediate tubercles in *Protagonodon* have coalesced and nearly form crescents. The external cusps of these superior molars are depressed and not as conical in section as in the Puerco *Creodonta*. The lower true molars of *Protagonodon* are sextitubercular, but differ in form from those of most of the *Creodonta* by the fact that the anterior portion of the tooth (trigonid) is not raised above the posterior (talon). The cusps of the lower true molars, as in the case with those of the upper molars, coalesce and form continuous tracts of worn enamel; this applies particularly to the posterior limb of each crescent. Lastly, the upper premolars in *Protagonodon* are not yet known, but the lower teeth of this series are well preserved and shows them to be absolutely simple in structure, consisting of a cone with slightly enlarged heels. In some specimens there is a trace of an internal cusp on the last lower premolar.

The characters above adduced as pertaining to the dentition of *Protagonodon* approach closely those of the earliest known American Artiodactyle, viz., *Pantolestes* from the Wasatch Eocene, I would suggest accordingly that *Protagonodon* may stand in ancestral relationship to this genus.

I do not agree with Dr. Schlosser in deriving the Artiodactyles from any of the known Peripitychidae, as the latter group has been defined by Professor Cope. In nearly all of the Peripitychidae the premolars are highly specialized and are not adapted for further evolution. Professor Scott, in his very valuable paper on the "Creodonta," only recently published, has subdivided the genus *Mioclaenus* Cope into several new genera, limiting the latter genus for a few species only; the type being the *Mioclaenus turgidus*. The structures of the premolars in *Mioclaenus* are more like those of some of the Peripitychidae than the *Creodonta*, and consequently Professor Scott believes that *Mioclaenus* is a condylarth. Other than the genera already mentioned as probably having been persistent types, I would intimate that *Mioclaenus turgidus* may stand in ancestral relationship to some of the White River bunodont Artiodactyles (*Leptocherus*). The following phylogenetic scheme may illustrate the affinities proposed in this paper:

Perissodactyla. Bunodont Artiodactyla. Selenodont Artiodactyla. Amblypoda.
 Euprotogonia. *Mioclaenus*. *Protagonodon*. *Pantolambda*.

DO THE LEAVES OF OUR ORDINARY LAND PLANTS ABSORB WATER?

BY EDWARD A. BURT, EAST GALWAY, N.Y.

CONFLICTING answers have been given to this question. Hales, Boussingault, and Henslow concluded from their experiments that leaves do absorb water; other investigators have failed to obtain such positive results, and have been inclined to doubt absorption. Furthermore, the theory that the transfer of liquids is largely accomplished through differences in density of the liquids in the plant caused and maintained by transpiration from the leaves—this, by giving a sufficient function to the leaves, has probably deterred investigators from entering upon an inquiry that promised only negative results, and that was beset with difficulties in carrying out. Yet a moment's reflection shows us that during the growing season of several months in each year, our vegetation is covered with dew night after night, and often when periods of drought prevent the plants from receiving an adequate supply of water through their roots. Does it not seem probable that plants are able to use the dew which covers their leaves?

Under the direction of Professor Goodale and Mr. W. F. Ganong, the writer has been recently carrying on a series of experiments in the botanical laboratories of Harvard University to determine—

(a) Whether it is probable that leaves do absorb water.

(b) Whether the conditions under which such absorption occurs, if it does occur, will not afford suitable ground for more special investigation later on.

Some of the results already reached seem to justify a preliminary publication.

Can Leaves Absorb Water?

To decide this, young branches of *Diervilla grandiflora*, common house geranium (*Pelargonium*), and *Mesembryanthemum* were cut from the parent plants while in full leaf. The clean-cut ends of these small branches were then dipped into a waterproof varnish—Brunswick black—so as to completely cover the cut ends and the sides for an eighth of an inch up the stem. The branches were then allowed to lie on a table in the laboratory—temperature, 70° F.—for a time until wilting occurred. They were then weighed, sprinkled with water, and shut in a botanist's tin collecting-box for from 16 to 46 hours. Having recovered their original fresh condition, the branches were then removed from the box and dried carefully from adhering water by exposure to the air of the room and by the use of blotting paper. They were then weighed. In each case there was an increase in weight indicative of absorption. The details are given in the following table:—

	Period of wilting	Weight of wilted branches.	Time shut in the box.	Weight of water absorbed.
	Hours.	Grammes.	Hours.	Grammes.
<i>Diervilla grandiflora</i>	3	12.12	16	0.36
Common geranium (<i>Pelargonium</i>)..	49	26.79	46	5.76
<i>Mesembryanthemum</i> (a succulent- leaved plant)	49	40.25	46	0.77

Henslow obtained absorption with cut branches in a large number of cases and under a variety of conditions; but as he did not cover the cut ends of his branches, it has been objected that the absorption in his experiments occurred through the cut ends rather than through the leaves. My experiments show that the objection was not well taken. We must conclude that slightly wilted leaves may absorb water.

Do Leaves of Rooted Plants Absorb Water?

Small vigorous-growing plants of *Ricinus* and of a small-leaved *Begonia* were used. They were obtained from the greenhouse in 2- and 3-inch pots. The pot and the lower portion of the stem of each plant were then inclosed in a covering of sheet rubber in the following manner: A small circular opening of less than half an inch in diameter was cut in the centre of a piece of sheet rubber of suitable size. The rubber was then stretched in the region of the opening so as to make the aperture temporarily larger. The pot was then slipped down through this opening.

Upon lessening the tension, the rubber contracted clasping the stem just below the lowest leaves. With a stout thread the rubber was then wound firmly against the stem for a sufficient distance to make a close contact of the two. With its centre suspended from the place where tied about the stem, the rubber now hung down covering the pot loosely and completely concealing it. The lower portions of the rubber were now gathered together underneath the pot and firmly tied together with strong cord.

A thrifty young *Begonia* plant with its pot so covered had its leaves thoroughly sprayed with water by means of an atomizer at 6 P.M. It was then placed under a large bell-jar in an atmosphere made and kept damp by wetting the inner surface of the jar with water and by suspending in the jar two large sponges dripping wet. With its leaves wet, the plant was kept in this damp atmosphere in the dark during the night. In the morning it was removed from under the bell-jar, dried carefully, and then weighed at 8.40 A.M. It had increased its weight 0.09 grams

during the night. This increase must have been due to absorption of water by the leaves.

At 8.40 A.M. the bell-jar was removed to a window space and the damp atmosphere was obtained within the jar as before. The leaves of the plant were then thoroughly sprayed again and the plant was placed under the jar and left there in a strong light during the day. From time to time, as the water began to disappear from the leaves, they were resprayed. At 4 P.M. the plant was removed from the moist chamber and carefully dried. It was then weighed and showed a loss in weight since 8.40 A.M. of 0.41 grams.

On repeating the experiment with the same plant, the increase in weight was 0.04 grams during the night—from 6.15 P.M. to 8.20 A.M. From 8.20 A.M. until 2.30 P.M., there was a decrease in weight (transpiration) of 0.23 grams.

But was the increase in weight during the night in these experiments really due to absorption of water by the leaves? May not the moist air surrounding the plant have passed through the rubber covering and deposited some of its moisture upon the earth or pot, thus giving absorption by the earth rather than by the leaves? Such an interpretation of the experiment is forbidden by the condition of the interior found upon opening the rubber covering at the close of the confirmatory experiment. (That condition was not *precisely* known while the experiments described were in progress, for the plant had been subjected to experiments for several weeks, during which time its growth had made it difficult to give to the plant amounts of water exactly equal to the amounts transpired from day to day). Upon opening the rubber covering, the earth in the pot was found wet to the touch, the pot was wet, and the whole inner surface of the rubber covering was wet. In this condition of things, the greater movement of the water must have been from within the pot *outward* through the rubber to its dry outer surface and the drier—comparatively drier—air surrounding it in the moist chamber. If such a movement of water did occur, its effect was that of diminishing the weight of the plant during the night. We must regard absorption by the leaves as the cause of the increase which really occurred.

How potent a factor light is upon the functions of the plant, is readily seen by a comparison of the changes in weight during the day in these experiments with the changes during the night. At night, in the darkness, absorption perceptible by the balance occurred; during the day, transpiration predominated although the leaves of the plant were kept wet with water and in a moist atmosphere. Is it not possible that some of the failures to find absorption by leaves may have come through nice quantitative experiments having been carried on in the daytime, as would be the more convenient?

In conclusion, the experiments so far as they have been carried, seem to show—

- (a) That leaves may absorb water.
- (b) That leaves of growing plants do absorb water during the night when they are wet with water and in a moist atmosphere—i.e., under dew conditions.

INDIVIDUAL SKELETAL VARIATION.

BY FREDERIC A. LUCAS, U. S. NATIONAL MUSEUM, WASHINGTON, D. C.

THE subject of individual skeletal variation is one of considerable interest, to the morphologist from the hints it may give concerning lines of descent, to the systematic zoologist from its bearing on the specific units of classification and to the vertebrate paleontologist since he must mainly rely upon more or less fragmentary skeletons for the determination of species.

External variations are readily perceived, often easily accounted for by known conditions of environment, but the question how much may the skeleton of a given species normally vary is by no means easy to answer.

Unfortunately the problem is rendered all the more difficult from the fact that the large series of specimens necessary for its solution are seldom available, so that characters may be considered of specific value, or, on the other hand, as mere abnor-

malities, when they are really normal variations or, perhaps, due to changes brought about by age. The following notes are somewhat desultory in their character, but they are based on the observation of considerable series of individuals of the various species referred to, and are brought forward as suggesting the existence of a large amount of individual skeletal variation.

In the report of the U. S. National Museum for 1887-88, the writer gave at some length the results of the examination of a large series of bones of the Great Auk, a series that was particularly interesting from the fact that it represented adult individuals from one locality and one epoch, so that any variations might be considered normal, and not due to differences of environment, or to modifications that might gradually come about in the course of time, even were there no change in surrounding conditions.

It may be briefly said that the long bones were found to vary to the extent of one-fifth of their length, but that the most interesting variations in the skeleton were the tendency to develop a ninth, extra pair of ribs and the frequent presence of a small tubercle on the tarsus, just where a hind toe would be located.

Very nearly one sacrum out of every seven possessed facets, showing the former presence of an abnormal number of ribs, while but one-twelfth of the tarsi showed the little tubercle referred to.

Professor Newton found almost equally great variability in the bones of the Dodo and Solitaire, birds of unusually restricted habitat, but this he ascribes very largely to the fact that the remains examined probably represented individuals from very different epochs.

Among mammals the Orang seems to exhibit an unusual tendency to variation, and a series of crania of this animal shows many individual peculiarities.

Doubtless these are shown by other portions of the skeleton as well, but, at the time a large series of Orangs was available, my attention was directed almost entirely to the skull, and it can only be said that this species has considerable range in point of size, adult males being from four feet to four feet eight inches in height.

The Orang is a striking example of the cranial changes brought about by age, these being so great that four species have been founded on characters which a sufficient number of specimens shows to be due to age alone.

Apart from these it may be said that the foramen magnum has hardly the same shape in any two skulls, while the nasals vary as much, being sometimes long and narrow, sometimes short and broad, and in one case quite absent.

The shape and size of the orbits is very variable and they may be close together or some little distance apart. At the same time the supra-orbital ridges are often larger in rather young Orangs than in very old individuals.

A rather curious feature in the Orang is the tendency to develop an extra molar, the normal number being three, as in man. Usually this additional tooth is in the lower jaw and unpaired, but one jaw possessed four perfect molars on either side.

Our Mule Deer shows great cranial variability, both in size and proportions, and while typical skulls of the Mule Deer, the Columbia Deer, and Virginia Deer may be recognized at a glance, in many instances, where the antlers have been shed it requires careful examination to distinguish the skulls of the species apart.

The tendency to develop an extra pair of ribs is not very uncommon among birds, and, as we know, is occasionally seen in mammals, where it may take the form of a short pair of ribs on what would normally be the first lumbar, much more rarely a rib, or pair of ribs, on the seventh cervical, and sometimes that of an unpaired rib on the first lumbar.

In cases of this last mentioned variant the odd rib is usually longer than when an extra pair of ribs is present.

The true sacra of birds are ordinarily devoid of parapophyses, in fact this is one of their distinguishing characteristics, yet among Cormorants these processes are not infrequently present and I have once observed them in a Gontucker.

Although it is not uncommon to meet with an additional pair of ribs among birds, any lessening of the normal number is very rare and only once has such a case come under my notice, this

being a common Cat Bird in which the haemaphysia had disappeared from the first dorsal rib, the true ribs being thus reduced to five in number.

It is quite possible that reduction in the dorsal region has been carried almost to its utmost extent among birds and existing facts seem to support this theory.

Among the highly specialized Passeres, the normal number of ribs, counting as the first the most anterior that is connected with the sternum, is uniformly six.

Close to the Passeres stands the heterogeneous group of birds termed Picariæ, many of which are doubtless survivals of the ancient forms from which the Passeres have been derived.

If this be the case the line of descent of these Picarians is a long one and in many respects they may have undergone more modification than their more recent relatives.

Certain it is that in this group we find, with very few exceptions, those birds having the smallest number of ribs, sometimes only five pairs, and at least once, in our Night Hawk, only four.

In the Swifts, near relatives of the Goatsuckers, it is not asserting too much to say that we can actually see the process of rib reduction going forward, for among these birds we find many specimens with six pairs of ribs, rarely one with seven, and in the majority of cases six complete pairs of ribs and the lower portion of a seventh, and this lower rudiment is present in varying proportions.

Lower in the scale, among the Amphibians, the number of vertebrae is inconstant, even in such species as *Necturus* and *Menopoma*, whose pre-sacral vertebrae are fewer in number than in any mammal.

Necturus may have eighteen or nineteen pre-sacra, *Menopoma* nineteen or twenty, *Siren* forty-one, forty-two or forty-three, and *Amphiuma* sixty-four or sixty-five.

Variation in the number of caudals is, of course, to be expected, but in the long-bodied *Siren* and *Amphiuma* it may amount to as many as five or six vertebrae.

A curious variant has been noted in the sacrum of *Menopoma*, which Huxley, in the last edition of the *Encyclopædia Britannica*, describes and figures as composed of two vertebrae.

Unfortunately the specimen on which the figure and description are based was abnormal, for, like the Salamanders, *Menopoma* has normally but one sacral, and an intermediate condition, a true abnormality, may exist of ten vertebrae connected with the ilium on one side and one on the other.

It is evident from the instances just related that a considerable amount of individual variation in size, proportion of various bones, or even in the presence of certain bones, may exist in a given species.

Differences of size, unless excessive, are of little value, provided the parts retain their relative proportions and in judging of differences of proportion the question of age must be taken into account also.

Broadly speaking, variations are of two kinds, due to modifications of development or of structure, and the importance of any departure from a given type depends very largely on the answer to the question, to which of these two categories does the variation belong.

Modifications of development produce individual variations of size and strength, length of limb and power of jaw, modifications of structure—when constant—give rise to specific, generic or ordinal distinctions, as the case may be.

In the occasional extra molar of the Orang the extra ribs of birds, the tarsal tubercle of the Great Auk, and the varying number of vertebrae in Amphibians we have variations of structure that, being inconstant, have no specific value, and yet have a morphologic meaning of their own.

The extra molar of the Orang is probably a reversionary character, the extra ribs of the Auk and the little nodule occupying the place of the missing metatarsal certainly indicate an ancestral form with a longer body and four toes.

In the abnormal sacrum of the *Menopoma* and the five pairs of

ribs of the Cat Bird we have progressive variations, and these are of much rarer occurrence than retrogressive characters.

The parapophyses in the sacral vertebrae of Cormorants are teleological modifications, efforts to provide an additional brace for the pelvic walls of these strong swimmers.

The differences in the axial skeleton of birds and Amphibians indicate that variation in this region is not greatest in animals now possessing the largest number of vertebral segments, but in those whose embryology hints at the existence of more vertebrae in their comparatively immediate ancestors than are possessed by the descendants of these forms.

This would account for the frequent appearance of extra ribs in birds, the inconstancy of the number of vertebral segments in Urodele Amphibians, and the constancy in the vertebral column of mammals.

To conclude, many variations are reversionary in character, some progressive, and some due to physiological causes, most, if not all, have some definite meaning in their abnormality.

NOTES ON JAPANESE METEOROLOGY.

BY ALBERT S. ASHMEAD, M.D., NEW YORK, N.Y.

DESPIITE the humid climate of Japan, rheumatism is very rare among the natives, which is probably due to the practice of daily hot bathing.

The meteorology of Japan is exceedingly peculiar and of exceptional interest. As particular influences in the process of acclimatization may be mentioned, lessened, eliminatory activity of the lungs, increased activity of the skin, diminished cardiac circulatory power. A prolonged residence in the Japanese climate is productive of general physical relaxation, with increased susceptibility to cold. After a two years' residence in Japan, Europeans feel the necessity of wearing more substantial winter clothing, as the climate seems to have become harsher since the beginning of their sojourn. Any foreigner who permanently resides there and wishes to feel at ease must resort to the hot bathing of the natives; being in Japan, he must do as Japanese do. Europeans, on their first arrival, are very prone to rheumatism, and even perfected acclimatization does not do away with that propensity. The hot-bath habit is singularly favorable to perfect acclimatization; it, and also the customary and frequent hot tea, mitigates the depressive influence of the summer kakké months, the wet season of June, July, and August. Strange to say, in their national diseases, beriberi, there is an entire absence of perspiration; these patients perspire only in their last agony. One should think, after that, that the Japanese would consider baths as remedial in kakké. Strange to say, it is not so; they consider it only as an essential and, for them, very pleasant part of the toilet.

In kakké the popular verdict is, and has always been, that it is detrimental. The altitudinal is their most efficient treatment. Such a treatment is always, at least in our European and American experience, a dry one; dry air. It is not so in Japan; in their mountains, even as high as 8,000 feet above the sea level, you will find an increase of humidity, due to the precipitation from the volcanic peaks. Even in this heavy humidity, where they are endeavoring to cure a disease in which perspiration is suppressed, they do not give to the hot baths which are used there as much, but not more than in other not sanitary places, credit for any good accruing to the patients. And, in fact, if hot bathing contributed to the cure, such an influence would be observed at the sea-level as well as in high altitudes.

Of course, I cannot treat the question expressed here. Let me only say that, in my opinion, humidity has nothing to do, directly at least, with beriberi; it is not a climatic rheumatism. Its cause is the action of a carbonic poison in the blood, and that poison cannot be eliminated through the influence of hot water. Hot bathing, as I said, has nothing to do with it, either directly or indirectly. Indirectly humidity has, because it keeps the carbonic gases together and prevents their dispersion. The oxidizing influence of the pure air of the mountain heights has everything to do with the cure.

LETTERS TO THE EDITOR.

* * * Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The editor will be glad to publish any queries consonant with the character of the journal.

Bibliographic Work in Vegetable Physiology.

I AM on the point of making a suggestion to students of botany, chemistry, and more specially of physiology. I would be glad to receive notes concerning the literature of any question in physiology, in order to use them in my bibliographical work concerning the physiology of plants. Under the head-title of "Contributions from the Missouri Botanical Garden" a series of bibliographical papers will be published, treating of every question within the range of vegetable physiology.

Students of any college in the country could assist me a great deal, if they would inform me of their being willing to pick up occasional notes on this or that question. The bibliographies of *Inuline*, and of the *Tannoids*, both with special reference to the rôle played by these constituents in vegetable physiology, have already been issued. The question taken up at present is that of the *alcoholic fermentation*. Anybody wishing to assist the writer in preparing his bibliography on this subject by sending lists of references—all of which will be welcome—or by looking through a journal or other periodical, thus saving a little time for the writer, without much loss of time for himself, will receive hearty thanks, and will be mentioned as a contributor.

This note being submitted to the attention of all students of science as well as professional scientific men, I wish that students of colleges and universities would act upon it. Often students are at a loss as to how to do scientific work and contribute to general knowledge. Here is one of the departments where much work is needed. References might be taken in the following way:

1. Select some chemical, botanical, or physiological journal.

Begin with Vol. I., and go over the whole series carefully, noticing every place where the alcoholic fermentation has in any way been mentioned.

2. Write carefully: (a) Title of the paper, (b) name of the journal (for journals, see Bolton's Catalogue of Scientific and Technical Periodicals, 1665-1882, and his Catalogue of Chemical Periodicals, the first is found in any library, and was published by the Smithsonian Institution; the latter is found in the annals of the New York Academy of Sciences, Vol. III., Nos. 6-7, pp. 161-216, 1885, with supplement, *ibidem*, Vol. IV., pp. 19-22, 1887), (c) volume, page, and year.

3. Examine text-books and handbooks in which the question of the alcoholic fermentation is mentioned.

4. Examine also papers and works which do not bear directly upon this matter; sometimes interesting remarks may be found.

J. CHRISTIAN BAY.

Missouri Bot. Garden, St. Louis, Mo., July 18, 1893

A Plea for Botany in the Small Colleges.

The many pleas made for a better presentation of botany in the larger institutions of the country, have induced me to add a word for botany in the smaller colleges.

The present status of the science in these institutions is indeed discouraging as it is presented in their catalogues. The traditional term of botany given by an instructor in physics or chemistry is the common allowance doled out to the students. The conditions are, however, changing gradually, and chairs of biology are being established in many of the smaller colleges, whose incumbents are occasionally botanists. As a teacher of botany in one of these colleges, the writer wishes to add a plea for the introduction of botany in its proper proportion into the college curriculum.

The character of the work of the college is somewhat different from that of the university in that its courses are necessarily briefer and less specialized. Their students more frequently

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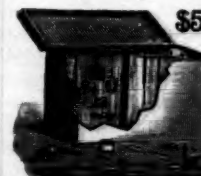
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pursue a course which leads to the so-called general education, and the question naturally arises, what place has botany in such a scheme of equipment for life?

To the average college graduate few if any of the sciences can be said to be directly useful, they profit him largely in the breadth of view which they give, and the pleasure they are able to furnish in their contemplation or pursuit. In these latter respects one can scarcely conceive of a science which would rank higher than botany. There are certainly no phenomena which are met with more frequently by the non-professional than those which appertain to plants and plant life. Without becoming sentimental one may say with truth that to one who has an intimate knowledge of this field of nature the world around us takes on a new aspect, and new truths can be discovered and added daily to the fund already acquired. But it is on account of the peculiar adaptability of botany to teaching, that the science should appeal to the smaller institutions.

That science is best adapted to teaching which is able to present its material at first hand for investigation, and whose truths are within the ability of the student to discover.

The material for botanical study is abundant everywhere, and presents problems in a measure peculiar to each region. The early stages of investigation in the science are not difficult and do not require expensive apparatus. The live teacher who sends his students to the field and not to books, will find in botany a science in which enthusiasm can be aroused and progress made without an expensive outfit.

In the planning of our college courses in botany one must needs bear in mind two classes of students, those who are to go on with the science and those who pursue it as one of the elements of a general education. It is the former class who too frequently suffer in the average college.

The courses should be given in such a manner as to give the student who wishes to pursue the science in a university a foundation which does not need repeating because it is antiquated or

abbreviated. In this way I believe the small colleges can be made centres of enthusiasm for botanical science, which will materially advance its teaching and its standing in this country.

It is to be hoped that botany will one day take its place in the curriculum of the small college as one of its most important constituents for the training of men. X.

AMONG THE PUBLISHERS.

"CAMP-FIRES of a Naturalist" is the title of a forthcoming book which sketches big-game hunting in the west from a fresh point of view. The author describes the actual adventures and experiences of a naturalist, Professor Dyche, of Kansas University, who has hunted from Mexico to the northern confines of British Columbia, pursuing grizzly bears, mountain sheep, elk, moose and other rare game. As an outdoor book of camping and hunting this possesses a timely interest, but it also has the merit of scientific exactness in the descriptions of the habits, peculiarities and haunts of wild animals. The author is Mr. Clarence E. Edwards, and the book is to be published immediately by D. Appleton & Co., with many illustrations.

—Professor Charles S. Minot's "Human Embryology" is announced to be translated into German. The translation is being made by Dr. S. Kästner and will be published by Messrs. Veit of Leipzig. The author has revised the entire work for the German edition and has made a series of changes and additions, which will render the translation practically a new edition. Among the changes is the making of a new chapter in the Introduction, giving a complete account of the external development and growth of the human embryo through all stages. References have also been added to important papers published since the original American edition was issued. The honor of a German translation has hitherto been accorded very rarely to American scientific works.

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First inserted June 19, 1891. No response to date.

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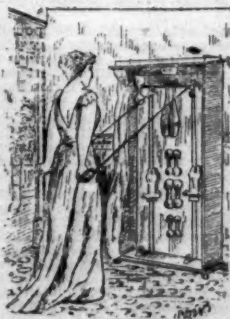
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